INTERVIEW WITH DR. ROBERT NAUMANN
INTERVIEWED BY STEPHEN P. WARING
3 JUNE 1991
UNIVERSITY OF ALABAMA IN HUNTSVILLE
HUNTSVILLE, AL

1. WARING: . . . personal background question. How did you get into the space business here in Huntsville?

2. NAUMANN: Well, Back when I was a youngster in high school or even grammar school, World War II was ending, and I was absolutely fascinated with the Manhattan Project. I read everything I could about it. I felt disappointed that I was too young to participate in that, but when I read that Von Braun was coming to the US and the rocket center business was moving to Huntsville, I said "By God I missed the Manhattan Project but I don't want to miss this one." I was a freshmen in college when Von Braun's team moved to Hunstville. My former next door neighbor had come up to Huntsville and had gone to work for [?9]. I got in touch with him and asked him if there was any summer jobs or something like that up here. He [?11] and gave me a summer job and I've been here ever since.

3. WARING: You weren't drafted like a lot of them?

4. NAUMANN: No, I volunteered.

5. WARING: That's interesting.

6. NAUMANN: I was a sophomore in college and I came up here and spent a summer in 1954. I came back in '55. I didn't spend '56. I worked for Hayes International in

Birmingham to see what the aerospace industry was like and I didn't like that. As soon as I graduated in '57, I trekked up here and have more or less been here ever since.

7. WARING: And you got your advanced degrees after you were here?

8. NAUMANN: Yeah, through the government. I did a lot of my course work here at UAH back then. I took off for one semester and went back to Tuscaloosa and finished my course work for my masters. Several years later I went back and finished my course work for my Ph. D. and did my thesis here.

9. WARING: Are you a native of Alabama?

10. NAUMANN: Not really. I was born in Illinois, but my folks moved to Birmingham when I was six. I sort of adopted the South.

11. WARING: Well, that's a sort of interesting background. Marshall was always involved with materials processing in one form or another. Certainly that was an important part of the Saturn project. Could talk about how that Saturn work contributed to later development?

12. NAUMANN: Well, I was really involved in materials business up until fairly late in my career. My training was in astrophysics and my first duties were, I was doing general design, in fact I did some design on SPAR 1, and did a lot of the orbital mechanics, a lot of rigid body motion, in fact I worked with Dr. Lundquist[?] very closely on some of the early orbital determinations and the satellite behavior, the rotation, rigid body rotation and that sort of thing. Then we got involved in nuclear weapons effects back during the Argus program. We were still under the Army in those days. Then later on, my first involvement

with materials was, we got involved in worrying about meteoroidal protection. That meant we had to worry a lot about material properties both with the striking particles and with the impact on the structure.

13. WARING: Were you working on the Pegasus?

14. NAUMANN: Yes, as a matter of fact I was directly involved with the Pegasus. I was the investigator for the [38?] experiment on that. Materials lab had a long history not so much with developing materials, but their major thrust really was certifying the materials that were being used and the various things that Marshall was involved in were indeed sound and what they were supposed to be. They did a lot of the certifications of the materials. Then of course when they started developing these advanced engines for the Shuttle, there were an awful lot of materials problems involved. A lot of the problem with hydrogen [44?], a lot of problems with [?45], and all sorts of leaks and everything else. [46?] seal for the oxygen and we had the hydrogen for the Shuttle main engines was a headache for many years. They had a very, very active materials program primarily from the point of view of testing materials and doing [?48] analysis of materials when the engines came apart of course we had to figure out what happened to them. I really didn't work in the materials lab. We sort of worked in parallel areas similar to the meteor work we were doing in the space science lab was considered duplicate of some of the work some people in materials lab were doing and that was a little bit of a source of friction between the two labs, bu not a serious one. The materials processing in space business really started three of four different aspects in the center. Certainly the very early experiments were done in the space science lab by [55?] and Barbara [55?]. They were working with [?] out here at Lockheed and also ?. They did probably the first experiments on how materials behaved in space. This was motivated because we were never involved in thermal design in those phases of spacecraft and one of the things that was of interest to the thermal design people

were these things called phase change materials. These were protective waxes that have relatively low melting points but have a fairly high heat of fusion. The idea being that if you wanted to keep a satellite on a certain temperature you could put a lot of this wax on certain areas and make a big thermal capacitor out of it. What happens is a lot of energy is stored up in the solidification of this wax. If the thing gets cold, the wax starts solidifying and gives off heat of fusion and keeps everything level. If it gets too hot, the wax starts melting and ? a lot of this, and you've got a lot of thermal mass there that could buffer the swings of the satellite going in and out of the earth's shadow.

15. WARING: These early materials processing experiments were largely supplemental to engineering work?

16. NAUMANN: That's correct. As I say, there were about three avenues of approach to that. One of them was in the thermal pull area and of course the interesting thing there was that well if you've a materials in the change phases on hour, you have to know where the solids going to form. Since these things have different densities their not going to do the same thing in space they do on earth. Some of the very early work was motivated by trying to understand the solidification of these waxes. That's how we got into the solidification business.

17. WARING: Now, was that designed for any specific project. Was that for Skylab, or Apollo, or . . . .

18. NAUMANN: No. Some of the waxes were really kind of an advanced development. Without anything specifically in mind but just a good way of doing things.

19. WARING: What date are we talking about here?

20. NAUMANN: This was in the 60s I guess - mid-60s. We had gone through the early explorer satellites and in fact the group that I was with in the space science lab under Heller at the time ? . We were involved in the thermal design of space craft - Explorer I and Explorer VII and several of the others. Of course one of the big problems always was that you have to design a system that would work in the shadow where you had no direct sun and then you had to work also in direct sun. There were pretty large temperature swings due to the radiation, solar radiation environment, that you had to work in. So, we were looking at ways to smooth out those large temperature excursions so that the inner workings of the spacecraft would be under a constant temperature. That was the motivation for these thermal phase teams that I described. As a matter of fact, Oak Ridge did a lot of work on those recently, well, more recently because of the energy business. ? they use these for insulation in housing to store up heat in the walls during the day and then it would release during the night. It was the same set concept. That was one thrust that got us into the solidification business. There were other things also. One had to worry about the ~~?~~ Propel management especially when ? and that nature because obviously if you have a liquid stage once you turn the thrust off and basically free float, the liquid isn't going to necessarily sit down at the delivery line and you've got to worry about now where is this going to go and how's it going to be configured. Normally on the Saturn stage we had ? motors which were small rockets which were fired before we could turn the main stage on to shove the propellor back down into the thing. For a lot of purposes we really needed to know how that liquid was going to configure itself in the tank. That was the motivation for doing a lot of the ? work. How do fluids behave and low G and when they partially went ? and partially fill the container. It's a rather interesting problem and calculous variations in the fueler ? won't go into that. Then of course there was the hygiene business that we started developing in Skylab getting ready for that. We had to worry how to make the shower work in space; simple things like wash dishes or make a waste disposal system, of

course toilets were always a problem and still are. It just don't go down like it's supposed to. All of those were motivations for understand how materials behave, fluids particularly, behave in space. The very first experiment that was done in a weightless environment was carried on Apollo 14. That was the one where Filamina and Banister did the first experiment trying to understand how surface tension could actually work in space. There were what they called a little suitcase experiments. It was a little package that fit under the seat of the command module. There was a liquid in a shallow dish that was heated from below. What they were willing to do was to look at the relative effect that the surface tension in the connection as opposed to what you'd see on the Earth where you'd have both buoyancy and surface tension. I guess you're familiar with ? connection type of arrangement where if you heat a liquid from below which is of course unstable, but nothing happens until you reach a certain what we call ? number or temperature differences. It depends on the height and other things. Then what happens is these cells start boiling up from the bottom, these so called Bernard cells, it also pours in the atmosphere. That's one of the reasons that these big convective thunderstorms form. That's driven by buoyancy driven convection. Then there's another effect also on the fact that the free surface up here on the top, the surface tension is lowered in most liquids if you heat it up. Now you have a system even in the absence of gravity, the fact that I've got a hotter liquid down here on the bottom and a cooler liquid here on the top, the liquid could be the lower energy configuration if the hot liquid were at the top because the surface energy would be lower. That makes an unstable situation also. The real question is when you have these overturning convections in shallow dishes of liquid which is the more dominant here on Earth. Is it the surface tension or so called ? convection after the Italian physicist who got the credit for it. It was really an English physicist Pearson that first ? it but somehow it's gotten an Italian name ?.

21. WARING: Just to stop you for a second, would you say that these suitcase experiments were more in the realm of basic science rather than sort of applied science for solving engineering problems like some of the other things?

22. NAUMANN: I would say there were more ? experiment. There was some, I would say the emphasis was more on the basic stuff because from a practical engineering point of view, you're not really that worried about overturning whether or not it's surface tension driven or buoyancy driven. That's an interesting philosophical question that needs to be answered, but the experiments were not really controlled well enough to be considered critical scientific experiments either. They were sort of, well lets see how fluids really to the ? and what we can learn from it. There was some other little bit more engineering oriented experiments also that had to do with the E transfer problems. Those were little cylindrical cells where there was a little wire in the center and you heated liquid in the center and then you had these little liquid crystal materials that change color when they change temperature. You'd look and see how the heat propagated out and whether that heat was being carried by convective lows or by purely diffusion flows. Of course you know that in the absence of gravity, it's going to be carried by diffusion and the presence of gravity it's going to be carried by convection. What we found in Apollo was the heat actually did not match what you'd expect from pure diffusion. That was the first hint that we really were kidding ourselves when we were talking about zero gravity because it really isn't. Apollo for example even though it was a free coast on the way back from the moon, the space craft was actually in a barbecue mode because they have to rotate the spacecraft slowly to smear out the sun on one side so it doesn't get to hot on one side. This light roll, the centrifugal force, or official gravity, if you will, was enough to upset the purely diffusive heat transfer on it. So that, in retrospect, we say, well, any idiot would realize that. But we weren't used to thinking in those days about these very, very small disturbances as being terribly important.

23. WARING: Zero-gravity?

24. NAUMANN: Yes, zero gravity was what everybody was thinking because it really felt like weightlessness. Nobody really sat down and did the rather difficult calculations to actually see that it wasn't actually quite zero, now what is it? We figured that, gee if it is four or five orbs magnitudes down it was for all practical purposes zero. It is really amazing how many connected effects still work even at those very, very low levels. We are just finding that out.

25. WARING: Were people at Marshall who were involved in the Skylab experiments drawing directly on some of the things that they had learned in these two cases?

26. NAUMANN: Yes, I think so, but probably not as much as they should have. Again, the reason for that being this fall line. The Skylab came about primarily...well there were several motivations...the biggest thing with the Skylab was that NASA wanted a first concept of the space station. So it was originally a wet workshop. That is what we called it when we were going to set up shop inside an SIV-B stage. Then some other people got to thinking that well, maybe it would be nice to do a little bit of science while we were up there so that we would have some reason to have to show for being there. So then they decided to put the Apollo Telescope Mount, the array of solar observing instruments. So we would do long-term synoptics and solar observing. That was the primary purpose for the Skylab. Then at a later time because of the interest in the environment that was coming up in the late sixties, it was decided to add the so-called ~~E-raphic~~ [?186] experiments with earth resources, where we put a battery of Nikon cameras, side-looking radar and other things. We were also going to use it for earth survey. Then kind of at the very last minute, the materials processing business emerged. I guess that was part of the

legacy from the Apollo. We did those Apollo experiments on [Apollo] 14 and [Apollo] 16, if I remember correctly. There was another cause that I might also mention, that Buddy Yates and, I think that it called for TRW on a regular bid, that was axis locations where they would mix things like paraffin and metal filings, or paraffin and some other things, low melting point materials. They were actually melted in a small furnace and resolidified. The idea being to see whether one could get filing dispersed phases of materials with various part density differences, which was one of the things that motivated a lot of the early concepts of the material science group. So those were also carried out as location experiments.

Do you have that book that I put together some time ago called, Early Experiments? That traces all of that.

27. WARING: Yes, I believe I have that and two articles.

28. NAUMANN: The Skylab experiments came very late in the Skylab game.

29. WARING: Why was that? Was it because NASA was not interested until after they found some unusual things from the Apollo experiments?

30. NAUMANN: No, I don't think that it was that so much. I think it was the fact that the interest in this was sort of pushed, both by the people in Space Science Lab...that was the Tommy Bannister people and the group under Jerry Arnett at the time. And Matt Seveil had a big push in that also. Matt worked over in the Test Lab. I would say that Matt was the "Father of Materials Processing." Tommy did some of the early work, as I say, from the more scientific view. Matt had a group of engineers over there. Heinz Mucher [?216] was one of the prime ones who had a lot of just creative genius, if you will, that came up with an awful lot of early ideas. They really pushed a lot of the more engineering aspects of it. I

guess one would have to say that these concepts began emerging in the late sixties. We were able to fly one or two things on the Apollo.

Then the Headquarter's people began to get interested in this about the time that Skylab was sort of in its last stages of definition. But [they] decided that this was a good opportunity to do some of these things, since we had a lot of power and were going to be up here a long time. So it was really through reactions of Jim Brett, I suppose, and his predecessor, Bob Nash at Headquarters, that were instrumental in getting some of those experiments on Skylab.

Some of these were very engineering and practically oriented. For example, the welding experiments that were done by Gene Cannon and the people over at the Materials Science Lab, was a tube brazing experiment that was done by Dick Foreman, that were really aimed at building large structures and let's make sure that we don't have any real surprises up there. If we really want to do some welding, do we really know how the weld pools are going to form? If you want to do tube brazing and things of that nature, it would be nice to have some experience under our belt before we do this. So those were strictly engineering types of experiments.

Then, through the advent of that, there was this big, kind of containment vessel that was added in there to do the welding. Then we thought, well geez, as long as we have that in there we could do some other things. There were some combustion experiments that were done by Steve Kinsey down at Johnson. Of course, after the Apollo fire instance, everybody was very interested in combustion phenomena in low-g and how does it really work. So they did some early experiments on literally just burning pieces of paper and pieces of this that and the other, to see how flames progate. They did some smoldering experiments. That started off a fairly major effort in combustion science at low-g, which is going on even today. As a matter of fact, they are duplicating some of the experiments that were done on Skylab at about the same level of sophistication! I keep asking these people, what is it new that you are doing? I don't get very good answers. (laughter)

So, along the same line then, there was a furnace that was developed by Ron Mezelski [?255] and the company at Westinghouse, which we decided to add in. He did a lot of solidification experiments.

So, as it turned out, even though all of these experiments were done in a fairly short time. I think that we had something like eighteen months from the time that it was decided to add these experiments to the Skylab until the hardware was actually delivered. Given what it takes in time to do things today, that's a pretty remarkable feat! That was before we forgot how to do things.

31. WARING: Before all the red tape came?

32. NAUMANN: Exactly.

33. WARING: In the 1970s, especially after Skylab, the field of Materials Processing in space seemed to grow and change...

34. NAUMANN: Well, it got hyped a lot, I think that is probably the way to say it. What happened was, you had only one man mission, following the Skylab because of the very large cutbacks in the space program in those days. You know, there were a whole bunch of Apollo command modules that we had built up in the Apollo Program. You know that at one time we were talking about going to the moon once a month. The VAB was built so that it could simultaneously process four vehicles so that we could fly twelve a year. Then we suddenly realized that the Saturn V's cost $250 million a piece and that we would quickly go broke if we started launching those things at the rate of once a month. So a lot of those Apollo modules that were originally going to be dedicated to so-called "Apollo Applications Missions" that never happened. The only one of those that did happen was the Apollo/Soyuz project, which was the joint Russian venture back in the age of detente.

So what we did was that we took some of the things that we learned in Skylab and transferred those over to the Apollo/Soyuz Program. Now, I have to tell you that the resources that we had available to us on the Skylab were considerably better than what we had on the Apollo/Soyuz in terms of power, time, stability of the spacecraft. There was a lot of crew motion in the ASTP. So really the experiments that were done in the Apollo/Soyuz, were at least about a level of sophistication lower than what we really could do on the Skylab.

35. WARING: Just less sophisticated because of all the limitations?

36. NAUMANN: Exactly. Also we really didn't have time to take any real advantage of what we had learned from the Skylab, because the mission was close on the heels of Skylab. There were only about two years between the two flights. So that means that we really had to have most of the things designed and everything in place before we really had the results back from Skylab. It takes months to analyze some of these materials and figure out what is going on. By that time it is too late to bring changes for the next mission.

That gave us the first battery of data that we had. Then we had that long hiatus in manned spaceflight until the shuttle came along. It was almost eight years, I guess, wasn't it? To keep interest in the program, there was a ~~Saturn~~ Rocket program that started, the so-called SPAR program to Mars, something like six or seven, I forget how many SPARS, with rockets. But this was even a degree less of sophistication, because now you had even less power, far less time and no man involvement to fix things or change samples, or what have you. So we were sort of going on a downhill slope in terms of capability as the years went by.

37. WARING: This was trumpeted as being a grand project.

38. NAUMANN: Oh yes. Now the other problem is, that you have to remember to put into perspective was, that NASA was literally fighting for its life in those days. You had a lot of people saying, "Gee, you spent all of these billions of dollars going to the moon. What has it gotten us? It hasn't solved hunger, it hasn't solved Viet Nam, it hasn't solved lots of other things." So there was big [?314] of technology that kind of felt that technology let us down in Viet Nam. All those sophisticated weapons that we had didn't really work all that well. We lost the war anyway. The environment became a big issue and science was seen as a bad guy who was creating a lot of these environmental problems. So there was this big back lash of anti-technology. NASA, of course, was looking for its next big project to kind of keep its whole act together and the Shuttle was it. The Shuttle was, I guess in those days, what the space station has sort of turned into today, unfortunately...so as a result of that, there was a tendency, a motivation, if you will, for certain high-level officials to try to find any reason that they could possibly think of why we simply must have the Shuttle. Of course one of the things that they seized upon as having great benefit to society was this so-called "Materials Science in Space Processing." This was going to be the next industrial frontier. We were going to make [?330] ball bearings and other lot of total nonsense stuff that, unfortunately, got hyped up. Also unfortunately, the credibility with the scientific world was just about zero, because anybody with a grain of sense knew that, first of all, we could make perfectly good ball bearings on earth and secondly, even if you could make them a little bit better in space, that certainly wouldn't justify the cost of doing it in space. So, you didn't have to be rocket scientist... I was going to say, you wouldn't have to be a Ph.D economist to figure that one out! Even I, who know nothing about economics would say, "Hey, this is dumb!"

The point was, there was a lot of interesting things that possibly could be done. Of course the other one, I think that really got seized upon, and was rather fortunate, was the societal benefits. That was back in the days when we were trying to save society. As a matter of fact, there was even a directive that came out of headquarters, I don't know if it was exactly written or not, but simply verbal, that was from...let's see, at that time there was Office of Applications that was pushing this space processing business and I don't remember who the director of that office was, ...well, anyway, this guy was a real zealot in terms of applications for the shuttle. We were asked by him to come up with some scheme that would save ten thousand lives, or maybe it was a thousand lives, that we could equate to each shuttle launch. This was the way that we were going to justify to the public. Holy shit, you have got to be kidding! But anyway, one of the things that did kind of look interesting, was this idea of electrophoresis ~~electric foresis~~ [?362]. This was something that we had been kicking around for a long time. In fact, one of my good friends and colleagues, Bob Snyder, was directly involved with this. He came to NASA primarily because of the interest in this. He came, this was his first assignment. We had brought in Jeffrey Seaman. At the time I think Jeffrey was a Professor at the University of Oregon. Very, very competent biophysicist. He had done a lot of separation techniques and things of that nature. We brought him in as a visiting scientist...he took a years sabbatical to come work with us. There was a small electrophoresis experiment that was suppose to be performed on Skylab and kind of got kicked off at the last minute because we had some stress erosion problems in the Plexiglass and didn't qualify. The thing was rebuilt and flown on the Apollo/Soyuz. That produced some results that, I thought, provocative, but a little hard to really quite reconcile. The idea here...well, it did several things, but...used processing with zone electrophoresis, where you take a tubing material, put electrodes at either end and some frozen cells, were actually frozen in a little disk form that was inserted at one end of the tube. So, when you got into zero-g, that disk was taken out of the freezer and inserted into one end of the long tube and allowed to thaw. Then an electrical field was turned on. The idea being that the cells then migrate in the presence of the electrical field that was set up in liquid and they would migrate at a speed that was dependent upon the surface charge of a cell and the drag of the cell moving through great beet [?393]. The idea here being, that this was a way to separate cells...the hope for was this was a way to separate cells that had different functions, because the surface charge related to what was going on in the cell or what type of cell it was, so some would go faster than others and so forth. Hopefully, then we would get a spread of cell type over a period of time down the tube. Then after you run this thing for a certain length of time, you had a thermal electric cooler that would then freeze the tube in place, so that the cells would remain frozen and it was stuck into a cooler of liquid nitrogen and returned to earth. Then, of course, after you returned to earth, you could slice the thing up, slice the ice that was formed, pull out the cells in segment and stick them in a culture medium and grow them up and see what they did. The idea was that if you could separate cells according to their function this way, then you would have ways of maybe getting cell populations that would do certain things. Well the biggies in those days was the whole business of [?411]. Yolokindase was an enzyme that was made by kidney cells, fecal kidney cells. It was known to have very nice anti-clogging characteristics, it actually dissolved bloodclots after they were formed. At that time there was nothing else that would do that. Yolokindaze could be made, but was very, very difficult. It could be extracted from urine, but it took gallons and gallons and gallons of it to get a small yield. So the thought was if you could isolate the cells that would produce that particular enzyme, culture those cells, put them on a production medium, then we would have a way of making yolokindaze viable from the point of view of commercial production.

39. WARING: NASA headquarters was pleased about that?

40. NAUMANN: Yes, that was something that was very excited. There were a couple of other things. One of the other things they did was they took pituitary cells that produce growth hormone. There was an effort to isolate human growth hormone producing cells for the same reason. There was also an effort to isolate B&T lymphocytes. The idea being there...this was really before we understood a lot the immune system. The thought was that we could possibly use these lymphocytes, or somehow shut these lymphocytes down...I

never really did get the connection, but it had something to do with kidney transplants and the rejection mechanism of trying to manipulate the human response to prevent rejection of organs. Although I never really understood how separating B&T cells was suppose to do that!

41. WARING: In this electrophoresis experiments, that involved Marshall stepping outside of some of the hard sciences and engineering sort of experiments into life sciences experiments. Did that present any problems with people in Houston?

42. NAUMANN: Yes, it certainly did! They thought that was ~~there~~ their baby. Yes, they certainly did and that is still a bone of somewhat contention, although I don't think...well there are still some ruffled feelings, I guess over there. That was jealously guarded by Marshall as being theirs and it was jealously sought after by JSC as something they should be doing. There was not a lot of cooperation. It was a strained relationship, let me put it that way.

43. WARING: And how was the dispute settled? Did headquarters say, "Well, we are not going to intervene?" and then put [?461] at Marshall or has it still lingered on and never been settled.

44. NAUMANN: You wouldn't believe some of the stuff that went on there. They wound up funding Houston to do it for the simply reason that Houston owns the Shuttle and kind of told headquarters, "Look guys if you want to fly the damn shuttle, then you are ~~going~~ to sponsor this program." So that is sort of really... I think that the word 'appeasement' comes to mind. Both centers wound up getting involved in it.

What happened on Apollo/Soyuz was that we did do some of these. The cells came back and they sliced them up and put them in culture. Sure enough, a couple of the little slices on the fecal kidney cells, did seem to produce somewhat more yolokinaze then the other ones did. This got everybody really excited because they said, "Hey look what we have done here. This is something really great." Well, that is all well and good but then the next question would be, "Are there ways to do this on the ground." Of course electrophoresises is a very, very well understood and advanced technology from the point of view from earth of separating out proteins because...in fact this is a more standard method of separating proteins for analysis. We use a gel actually. You can put a protein solution on one end of the gel, you put a field on it and the proteins migrate across the gel. Then you can stain these things or do certain things with them and look at the spots on the gel or the bands on the gel and you can identify certain proteins. But cells don't go through gels very well, because of their small pore size. So free-flow electrophoresis was not as advanced on earth, but there were ways of doing it. There were several machines that were developed, or had been developed by Dalgin, Han Ingram, several others had developed these. We had several of them in the lab and these used a curtain fluid, a so-called continuous flow of electrophoresis where you put a sample streamer on the top, it goes down through a curtain into this buffer solution with electrodes on either side, so the cells then are swept across as they go down and then they are collected in the tubes down at the bottom. Presumably this does the same thing as the zone electrophoresis did in Apollo. So naturally...this is, by the way, about the time I really got into this game in a big way.

So we looked at the Apollo/Soyuz results and said, "Geez, these are interesting, but why can't we do the giggleyes [?507] on earth?" So we had machines that were built by Bagely and machines that were built by Hondly with everything else. G.E., at that time, were heavily involved in this, because they were looking for building major electrophoresis hardware for the spacelab. So they were busy working on it. I kept saying, "Well, can't this duplicated on earth?" Well, I would get all sorts of funny answers, "Well, it just can't. Don't ask why. It only works in space." Well, let's see how well it works on earth. We never really could duplicate the results on earth. Everybody said, "Well, there is all this convective mixing and so forth that takes place and stirs it all around." Well, we could put in sample particles and evaluate how much convective mixing there is and then spread it the pans a little bit. But there certainly was no evidence that the space electrophoresis had any sharper resolution than that on earth. So again, it was heavily publicized with an aurora of smoke and mirrors, or moving fecal matter around, you might say. Or as my friend Roger Crouch use to say, "El Toro PooPoo!"

Another interesting political thing was taking place. This was all during the long hiatus of manned space flight. This was just something that had built up and NASA was really excited and counting heavily on this being one of the major players. Well, in the meantime, the science community had sort of heard another all this hype and they got a little bit irritated and enough criticism and objections had been raised to the point that NASA felt to protect themselves they had to have a blue ribbon space committee to look into this whole business on materials processing in space ~~and see~~ if it is worth the effort. So, they convened a blue ribbon panel headed up by John Duetch, who was at MIT at the time. Duetch, by the way, was an electrophoresis expert, or at least fancied himself to be, and this was the so-called "STAMPS Committee" (Space Technology and Materials Processing in Space).

S: Yes, I have read about the panel.

45. NAUMANN: So anyway, I had just gotten directly involved in the program. I had taken over the division that was devoted to what we called Space Processing at the time. John Carruthers had just been brought in from Bell Labs to replace Jim Brad by Hal Loveless. Hal at that time was Deputy Administrator at NASA. John was really a first-rate material scientist. He had a very thrilling history at Bell. He was actually Canadian by birth. He had studied under John Glover at ...Glover was one of the very early pioneers of the material science business. John was primarily brought in to straighten out some of the

hype that we were really guilty of to overset the program. I guess I was sort of brought in for the same reason. So we really started asking some pretty hard questions.

Another interesting sidelight behind all of this. There was a very renown fuel mechanists, also from the National Academy, Dr. Simon Ostrech. Simon had been a consultant for G.E. on their electrophoresis project. Si was an interesting guy. I liked him a lot. He was an irascible old sort, but he is pretty bright. His sage advice to General Electric was, "Well guys, you have got this thing all wrong. What you are doing is flowing the liquid from the top to the bottom. And, you are heating liquid by passing current through the buffer, so you have an unstable connective situation on your hands. If you had any damn sense, you would turn the thing upside down and all your problems would go away." Now, personally, this is thinking like a fluid scientist, it turned out to be a perfectly good reason for running it the way they ran it. Si never asked what would happen if you tried it the other way. But, he has made statements that follow the STAMPS Committee, that NASA was totally stupid, that we had been investing all this money into something that, if we had known anything about connective flow that we would have realized that we were running the thing upside down, and all we had to do was to turn it over and all the problems went away. Which of course the STAMPS Committee said that NASA is totally stupid because they are running the machine upset down and all they have to do is turn it over and all the problems go away and look at all the money they have thrown away, so this is total nonsense, you shouldn't do it anymore. So this is the level of thinking from the National Academy! It really makes you proud! So the STAMPS people came in with a very scathing report. They simply said, that "Well, this is interesting stuff. It ain't going to make any industries in space. It ain't going to make any money for anybody in the foreseeable future." But I don't think that you have to be a rocket scientist or Nobel laureate economist to realize that. They did indorse some of the things that we were doing in fluid science. Some of the solidification stuff, they said, may have some interesting scientific aspects of it. The electrophoresis, Si had really torpedoed that one. That is kind of interesting too, because right after he had made this pronouncement, or he had told G.E. this. Then he applied for a NASA grant to Jim Grant (this was before he left that job at NASA). I remember seeing a letter that Grant wrote back to Si that said, "Professor Ostrech your application is regret....[end of side 1 of tape]...to have to parachute into an area that you just bought. Si's idea was that he was going to tell these guys how stupid they were and then come back to NASA and get a lot of money to make it all right again. As a result, electrophoresis got a really negative report from the STAMPS people and basically, we did hurt for a while.

But we did push on in the other areas. Basically, what the STAMPS recommendations were continue the program for five years after the shuttle is flying on a routine basis and see if anything really emerges. If at that time, you have people that are willing to use the service, and we are not talking about fleet haul-up cost, but we are talking about people that are willing to commit some of their own resources to avail themselves of the service, operate more like a national lab does, where NASA provides your infrastructure to do it, but people then are providing their own resources to use this capability. If there is that swell in the program that would be a good thing to do and should continue, otherwise it should be dropped.

46. WARING: How did NASA respond to those recommendations?

47. NAUMANN: Well, I mean, they more or less had to pay lip service to it. But I have to tell you that four or five years after that, they basically forgot it. Of course several other things happened at the same time. First of all, the shuttle program was several years later in coming than was originally anticipated. The actual flights, where we actually did some material science work were few and far between for a number of reasons. Of course, with the Challenger thing, we were set back a number of years also. So, one could argue that we really haven't had five years of routine shuttle operation for the program to prove itself,

even though this recommendation was made back in 1975. Here it is 1991, and we still don't really have that much experience under our belt.

48. WARING: Do you think that the criticisms of the STAMP Commission and the assumption of authority by John Carruthers lead NASA to fund more earth-based materials processing research to prepare the ground, or more for the work in space?

49. NAUMANN: Well, surely. One of the criticisms that was made to, and very rightly, by the STAMPS committee about the program was the experiments that seemed to produce the most results were those that were most thoroughly researched on earth before they went up. A lot of those experiments that were done on the Skylab and on the ASTP were, what you might call, "try and see" experiments. On the other hand, considering the way in which the experiments had been developed, the fact that all of this had been done in eighteen months, we didn't have a hell of a lot of time to prepare. Not only that, but since we didn't really know quite what to expect, there was only so much preparation that you can do on the ground. Some of the experiments were certainly better prepared and better thought through than others. I think really that the whole thing is having a P.I. with a little bit of insight and being able to apply some logical thinking and a little bit of common sense to the development of the experiment is probably the most essential ingredient. But John did insist that a lot more preparation be done before flight experiments were done for the ~~simply~~ reason that flight opportunities were very expensive and few and far between and let's fly only the best was the philosophy.

50. WARING: Do you think that change in emphasis would be reflected in budgets if we could get budgetary information...would there be any way to get an actual numbered figure for earth-based work versus space work?

51. NAUMANN: Yes, I think that is certainly available. John had set aside a certain amount of budget...I have forgotten the code numbers on the budget, but there was a major separation in the material science or space processing or whatever it was called, S&AD, I guess, at that time. It was 9..I have forgotten the numbers, but anyway the leading number on the budget number indicated the flight experiments versus the ground-based. Ground-based was certainly increased. But you have to remember that the flight budget goes for preparation of hardware as well as for paying P.I.s. The P.I.s expenditure was likely to be very small in proration the hardware budgets were large. So the ground-base stuff that you do on the ground is still very small compared to the flight budget because it is just incredible cost to build to build flight hardware.

Carruthers did try to get some science into the program. Some really first-rate material scientists. A lot of the people before were just dabbling at the thing and thinking well, gee maybe we can build a better semi-conductor up there if we mix X,Y,Z together and maybe something magical will happen to it in space and we will come ~~x~~ to revolutionize the semi-conductor industry. It was that sort of thinking that went into some of these early experiments. Then on the other hand there were some other really squirrelly things going on because up until Carruthers took over, even after he took over, the program was originally in the so called Applications Office. They had to show some sort of economical cost benefit analysis before you did anything. This really gets squirrelly. There was a study. I still have the document. It was done by ECON I believe it was up at Princeton. One of these economic think-tanks. The question that was posed to them was isn't it better to concentrate on your [712] production, this was before the ~~Stance~~ STAMPS committee bombed out the [714] thing, or kidney transplants. First of all I don't know why it had to be either or but that was the way the question was posed. So this damn Princeton firm was getting something like $60,000 to go on and study this question. Would you believe that the results that came back were that it's really not economically beneficial ~~so~~ to save people from various heart conditions with your [718] because they're going to require continual care. They went through this analysis of the hospital cost and the loss of work time whereas if they had died they could be buried they could be buried for only $2500 dollars. Do you mean we're paying taxpayers money for this kind of study? Unbelievable. So John put a stop to this crap in a hurry. He just killed all the funding for this sort of nonsense. They also moved the program out of the Applications Office over to the OSSA to try and give it a little bit more scientific . . . .

52. WARING: Was that '77, '78?

53. NAUMANN: Yea, let's see. I would say '77. I believe that's right. We were still getting a lot of these damn studies that were done before and there were still a lot of people out running around doing these economic analy~~z~~ses. For example, one of the arguments, well gee the semi-conductor business is going to go to a twenty billion dollar a year business. If we could make certain semi-conductors in space capture just one percent of that market, hey that's two hundred million dollars. I mean look at this. We're going to make two hundred million dollars, guys. All you've got to do is capture one percent of it. Kind of reminds me of some of these real estate salesmen you get on television. The how to make a million dollars in a month by borrowing on everybody's credit card and then finding something that's incredibly valuable that some sucker's willing to sell for half of what it's worth and then you find some sucker that's willing to pay you twice for what you've paid for it. Yea, you'd make a million dollars if, if, if. You can find the reality in this is just nowhere. Anyway despite of all this nonsense, John tried to get some science discipline into program. I think he did a really good job. I was heavily involved in it at the time and John and I were very close friends. We worked very well together on this. We were off trying to make some sense out of the program and eliminate a lot of the height and the nonsense and focusing on some of the things that really did make sense.

54. WARING: When you were brought in, they centralized materials processing at Marshall?

55. NAUMANN: That's correct.

56. WARING: Could you explain why th~~e~~y decided to do that?

57. NAUMANN: I don't remember all of the details about it, but there was a, I guess it was really in response to Headquarters in a sense that Headquarters had decided to get more serious about the program, and put new management in and make a division out of it. Marshall felt that they'd really should have locks on the program and there was a lot of unhappiness at headquarters with the way Marshall was running things. Lewis was very anxious to get into the ~~be~~ game. JSC very much wanted to be in the life sciences. Lewis said "Hey we're materials and structures people here, we do research and materials we ought to be part of it." So, [752] to build a larger center constituency for the program. Marshall was desperately holding on to the thing and insisting Headquarters that they [754] because they invented it. I think the decision was made when they knew Carruthers was going to come on board or had some feeling that NASA was going to do that. What they needed to do was to get their own act together and you had this rivalry between space science lab, materials lab, and test lab. That was all kind of working in three different directions. We said Alright let's it all and bind it under one thins. We'll put it under space science lab since NASA's going to put it under OSSA. Marshall was just undergoing a reorganization at the time. I had been working at that time, up until that time, on contamination problems with the Skylab and some of the other things we were developing with contamination monitor. On the shuttle, that was my division's role and responsibility. I didn't frankly want to spend the rest of my career worrying about dirty windows in

spacecraft. I volunteered to judge. I would be the person in picking this thing up and running with it. That's when [768].

58. WARING: I read a little bit about how Marshall wanted to become the discipline, the lead center for materials processing. The headquarters was unwilling to approve that. Why? Did they just want to divide and conquer and maintain more control out of headquarters by allowing the centers to compete? Marshall was managing the SPAR flights right?

59. NAUMANN: That's right. I think there were several motivations for this at headquarters. First of all headquarters never liked the lead center concept. That's from too much of the Von Braun days when you had the very strong leadership at the center. You kind of told headquarters where to go when they felt like it. They wanted to assert, I think, a more leadership role. The other thing, I think they were looking for a broader base of constituency. To be quite honest there were certainly talents at both Lewis and Johnson and elsewhere that they felt ought to be brought to bear on the problem because they didn't see Marshall as having the monopoly on the smarts, and clearly they did not. What's worse is that Marshall really had no recognized materials scientist working for them at the time. Not only that but there wan no strong university support for Marshall in this area. In Lewis, you had CASE. At Johnson, you had Baylor and Texas and Bryce. At JPL, you had Cal-Tech and UAH was dabbling with it but they unfortunately hired two of their leading researchers because they were on these research grants and the grants ran out and Marshall was slow in getting the grants reestablished. Dr. Otto and Hanus Walter who were both at UAH were two of the leading scientists in this whole area well. Otto is working with the OPLR in Germany and Walter is heading up the microgravity program at ESA now. We had two of the really top people here and just because of the bureaucratic bungling both at University administration, at Marshall, and at Headquarters let them get away.

60. WARING: That doesn't sound too surprising to me.

61. NAUMANN: No. Why should you be surprised? That was I think the motivation and then there was some political pressure being surprised also being applied. Our friend [799] was petitioning headquarters to set up a space processing institution at CASE institute. Guess who was going to run it? That would work very closely with Lewis. There was a lot of pressure being put on headquarters to get that. JPL was very interested in handling this processing thing. Taylor Wang at that time was with JPL. He was a big advocate of this. Of course Cal-Tech doesn't do diddly-squat in this area but they always wave a Cal-Tech flag. Langley was interested in doing some high vacuum stuff. They had a couple of experts that they had brought in to Old Dominion College in support and also the EPA. There was a lot of push to get more involvement in it. I think that was behind that and again I think part of the motivation of trying to put all this in a science division in Marshall was to bolster their position in some way, to say, "Hey look Headquarters, see what we are doing? We are really serious about this, too." So that was a lot of the motivation.

62. WARING: Another...I have a whole series of questions that straddling all of this stuff that you have been talking about. Could you tell me about the drop-tube, the drop-tower, how did those get built? Who built them at Marshall?

63. NAUMANN: Well, the originally, the drop-tower was built back during the Apollo days, I guess, or pre-Apollo days. The motivation, again, being the whole question of the development of management. Lewis was doing a lot of work in their drop facility and Marshall was also doing work in their facility. So that was the big drop-tower, that's the big twenty foot long, eight foot in diameter, however the big the thing is. Its like a big bomb that goes down the track. So there were a number of experiments that were done in that

during pre-Apollo days. Now that had to be disassembled while we were bringing the Shuttle in for the dynamic testing and the dynamic test-tower, because they had to take the whole side of the building off to get the shuttle in there. It was a full-stack and that was where the drop-tower was. After that testing was over, we decided that we really needed to reactivate that. Unfortunately, we never really got that operation going well. There were a lot of problems with the fact that was under one organization that really didn't provide the right kind of support for it.

64. WARING: What was that test...?

65. NAUMANN: Yes that was test [Lab]. They insisted that they have test people over there to run it. But they were never available to make drops. There was a very cumbersome mechanism for having to operate it because it had telemetry system that had to involve three or four other buildings and people, so it took a crew of a number of people to actually make it work. Those were never available when you needed it. But to be honest, all of those little difficulties could have been overcome. The big problem really was that there wasn't that much interest at Marshall in developing experiments to market. So there wasn't really a strong enough push for that.

66. WARING: So, you are saying that is not used?

67. NAUMANN: It has been basically reactivated. In fact, Bill Cauther here at the university is the only experimenter recently that has used that. His grant ran out and he finally decided that he didn't have enough time with this drop facility to see what he had to see.

68. WARING: Could you give me a date for that? Early eighties, mid-eighties that they reactivated that?

69. NAUMANN: Probably more recent then that. I would think late eighties, 1986, 1987, 1988 timeframe. It was only a couple of years again. More like 1988 probably. It was never really reactivated. I mean, it was reactivated for specific experiments. I guess we maybe averaged a drop every three or four months, probably less. I think that we probably did our first drop over there...I tried real hard to get that thing reactivated, but we ran into all sorts of snags. I guess we probably, boy, this is really fuzzy, but I would think that we probably got the thing up and running around 1984, 1985 timeframe and were able to do drops. Bill ran a series of experiments over there. It kind of all petered out around 1989. We decided it wasn't worth it. Plus the fact that Lewis has a very, very elegant drop facility, which I still think is more complicated than it was here, but they made very good use of it, so we just sort of felt that this....

Now the drop tube is another good story. That was really Lew Lacy's baby. Lew at the time was one of my branch chiefs that worked in the solidification branch. Lew was very interested in the undercooling of certain alloys, particularly he was very interested in superconductors. That was before the barium copper-oxide system that Hung K Woo came up with. Woo's working with the 8-15 compounds, which were germanium, niobium semi-conductors. At the time, those had the highest know transition temperatures of around 27 degrees, if I remember correctly. What Lew was trying to do, that was another stable phase, Lew was trying to trap that phase in formum form [?] using the drop facility. That was particularly timing tool, because History[?] at Bell Labs was actually the one that had discovered the 8-15 phase in these niobium compounds. Lew had been brought to headquarters to be John's deputy for this program. Then when John decided that he had enough fun at headquarters and left and went to Hewelett Packard and then INTEL, Histary took over the program. Until he decided that he had had enough fun in Washington and left and went to Bureau of Standards and then to Florida State.

There was a lot of interest at Headquarters in his work. By-the-way, Lew couldn't get the time of day from Jim ~~Brad~~ Brett. He never got funded. He had wanted to do this for a long time. It was one of those things where again, this was proposed as going to be some payload processing experiments to be done on the SPAR and later in Spacelab, where we would use an electromagnet levitator to do this work. Lew said, "Look, I can do this on the ground if I have a hundred foot tube." Bretts remark said, "Well, you will never be able to pump down a hundred foot tube and get a good vacuum in it." Lew managed to scrounge together some old pipe and he made the first hundred foot tube over in (Bldg) 4612. He and about one technician, I think, and a co-op student, put the thing in operation and got some really, really interesting results from it. They were, I think, probably the first undercool material well below the so-called "terms of limit." The limit that was predicted by ... that said you could only undercool to about twenty percent more than the normal freezing point. I think that Lew was able to get about twenty-five percent undercooling from the drop facility. Other people said that you would never be able to do that [?] material because you could never get all the nucleucation centers out. Lew showed that to be wrong at Airavestco's argument at Wisconsin. He really did get some interesting results.

70. WARING: What time did he assemble...what year did he assemble the...?

71. NAUMANN: That first drop tube went into operation very shortly after I took over the program which would have been around 1977 or 1978, 1979 timeframe. Then that was about the same time that we started thinking about putting the drop facility back in the vertical testing, the dynamic testing. I guess that was about the time the shuttle test of the Enterprise was finishing up. So, we were able then to start thinking about reassembling the

drop facilities. We found a whole bunch of stainless steel tubing over in one of the old test areas, that was originally suppose to be one of the oxygen delivery lines or something like that on one of the Saturn stages. It was a little bit out of spec and the tubing was just sitting over there. It was many thousands dollars worth of tubing, twelve inch diameter. So he was able to appropriate that tubing and got an assembly crew over there and put the facility together and made a working facility out of it. Fortunately, Ken, I think that is another one of those cases where we never really got the right kind of leadership and the right kind of support from either headquarters or Marshall management. Maybe we just didn't play it very well. I would certainly have to take some blame for that. But, we should have be able to make that a showplace facility. I mean a world class, where we had people coming in from all over the world coming in to use it. Because it was...in fact at the time it was the only one in the world. We had some interesting results on it. We really did not do a good job, I think, as selling that as a useful facility. It was, I guess, a case where maybe we just didn't try hard enough, or we didn't have the right support, we didn't have the right people doing it, or pushing it. But just really hasn't materialized. That is too bad, because that could have been...

72. WARING: Still a very fine facility or is it a little out of date?

73. NAUMANN: Well, it is still a very fine facility, it's just that we don't have that many users for it. It is puzzling too, you see, because the Japanese are duplicating facilities like that now. The Germans just put one in at Marina [?] and the French had put one in at Chernoble [?], both of which have capabilities beyond what ours has. One of the big problems that we had on ours, we never really got the instrumentation to work right. The other problem was that we never really could get the oxygen levels low enough. That is one of those things where I used to beat on people's heads, let's get some better instrumentation

over there, let's get some better pumps over there and so forth. This never really got done in the press of other more pressing problems, we just didn't get the right...

74. WARING: It was just another sort of science experiment or science project that suffered because of shuttle funding?

75. NAUMANN: Well, it wasn't so much shuttle funding, I think, it was just the fact that we all so many other things to worry with that had higher priorities. It was one of those things that gee, it sure would be nice if we had a better facility over there, but none of us, including myself (and I will have to take the blame for it), we just didn't put enough pressure on getting that thing, making it a first-rate facility. We could have done it. We had the resources. The resources could have been made available.

Another factor there, and I don't want to get too personal on this thing, we had a young fellow that was doing his Ph.D out of Vanderbilt was responsible for it. Of course, his main idea was to get a dissertation finished! So that was his high priority. He was doing a dissertation, and doing a really fine job on it, but, again, it did what he needed for it to do and with the press of getting his degree finished, he didn't really see any reason to worry too much to bring in other uses, it would simply interfere with his work. So, it was not handled as well as it probably should be. As I say, I certainly have to take some of the blame for that.

76. WARING: A lot of big science projects in the 70s, a lot of small science projects in the 70s, are going to all go in one chapter. So, I'll be showing connections so there will be some overlap here. One of the things about the SPAR program was that there was extensive involvement of university researchers and Marshall was managing these PI's from the Universities. Can you talk about the way in which Marshall managed their work? Did

Marshall manage the work of the Pi's differently from prime contractors at the same time or back in the Saturn years? What are your thoughts about that?

77. NAUMANN: Roger Chas~~ey~~ say [185?] was the project manager on SPAR. I admire Roger a lot. I think he's a very competent manager and he has a real feel for what Universities do well and what they should be expected to do and so forth. Marshall management in general~~ly~~ never learned to manage small projects. Their management culture, if you will, has been built around major Saturn type projects where the money's no object, where the best of everything has to be brought into it and everything has to be thoroughly documented whether it needs to be or not. When you try and manage a small university project and you only have maybe a few tens of thousands of dollars and some poor professor who's trying to do this on a very small short fraction of his time with no help, you really can't run it that way. Roger did have a lot of good feel for that and I think he had a very realistic management approach. I don't know enough about the individual relationships between the PI's and Roger although I think that it was a very good legal relationship there. Most of the PI's appreciated what Roger was trying to do, appreciated that he was trying to shield them from a lot of the bureaucracy. I think Roger personally took it upon himself some risk and probably wind up and do a lot of the work that normally the contractor would be required to do. I think Roger realized that there was just no way the contractor would be able to do that so he was able to find short circuits around that. That was probably one of the better examples of how small projects should have been managed and could be managed out at Marshall, but I don't think Roger got much credit for it.

78. WARING: Well, I'll talk to him.

79. NAUMANN: Yeah, you ought to do that.

80. WARING: I'll talk to him.

81. NAUMANN: It was one of those things where he was doing that but I don't think anybody really appreciated what he was doing.

82. WARING: Would you give me some good hints for things I can talk to him about? Well, let's talk about one big thing and then finish up here. One big question. This is pretty much, well I'll give you the question and you see what you want to do with it. In 1987 a AIAA report said that NASA's materials research was too dependent on the shuttle and arguing because they were dependent on the shuttle and the shuttle wasn't flying NASA was falling behind the Japanese and Europeans.

83. NAUMANN: As a result of the Challenger?

84. WARING: Yeah, the Challenger stuff. Has the shuttle helped to advance materials science research?

85. NAUMANN: In space or materials science research?

86. WARING: Materials science research in space? Do you think? Obviously this is more your opinion than you experiences as a NASA official. Do you think there's a feeling of materials science people of their involvement with manned space research, manned space flight has been less helpful or more helpful?

87. NAUMANN: Well, I guess I'd have to say that the shuttle has been a blessing and curse. The blessing is that if you build something for the shuttle in the course of the materials science arena unlike the people that do astronomy and other kinds of research

where you can send your data back, we really have to recover samples for the most part. Either there are either a few things you can maybe learn by [246?] or preliminary data, but the proof in the pudding ninety-nine percent of the time was going to be in the samples. You had

to get it back.

88. WARING: You got to get it back.

89. NAUMANN: Another thing about manned space flight is that your chances of you getting it back are infinitely better than they would be on something else, the ~~[250]~~ LDEF being the case in point where something was supposed to be up six months and it was up six years. It was dicey as to whether you're going to get it back at all and fortunately we did, but we could have just as easily not. So, the idea of the manned operations from that point of view is very attractive. Materials science is a science where you've got a sort of merging technology and wait. You really don't know everything that you are going to encounter and you can't really anticipate everything that's going to happen. It's useful especially in dealing with some of these fluid things to maybe have a man on the loop or a person in the loop to observe and try to do things. So that's a really strong argument for . . . .

90. WARING: The flexibility of running the experiment in space?

91. NAUMANN: Exactly. Of maybe running something and seeing what happens and maybe being able to fix it or alter or trying something new. Or maybe with very benign materials, little temperature materials as simple as water, be able to evaluate different configurations and play some games with it and get some hints of how things might ought to be set up and then you come back and the way to utilize that in a more demanding experiment with less than adequate materials and so forth that you would never attempt

unless you did some of this playing around that requires your presence. All of theses things argued for command involvement. Bobby Dunbar will give you a very long dissertation of why man is absolutely necessary.

Those are all the pluses. The negatives are that the shuttle is an extremely complicated, temperamental and costly beast. We still haven't gotten over the Apollo Syndrome, the fire syndrome. We were just beginning to get a little more comfortable with doing things on the shuttle and of course, then Challenger comes along. We have all seen the aftermath of that. So, as a result, we have six more layers of qualifications and everything else that has to be done in other to get something on the shuttle. So it is really no longer possible to do something that is simple, cheap, straightforward and responsible. Spacelab flight, for example, major hardware has to be integrated. It has to be at the Cape to be integrated. They like to have it eighteen months before flight. They absolutely insist on having it a full month before flight, or eleven months is the bare minimum. So that means that anything that you put in there is going to stay buttoned up at the Cape for eleven months, which makes it very difficult to do things in rapid...everything has to be mailed down way ahead of time. The documentation requirements in order to get something qualified to go on a manned spaceflight...again, this is another thing that I can get on my soapbox about, but you have got qualification requirements that are different for different centers; that are different with different program managers. It is just an absolute nightmare of bureaucracy. Case in point: some of the simplest protein-crystal growth experiment that Charlie Bugg flies. Charlie had to have three complete sets of documentation for that in 1984. He has to have one set of documentation if he flies on the mid-deck, if there is no spacelab involved that meets Johnson Space Center requirements. He has to have another set of documentation for the same set of experiments to go on the same locker if there is a spacelab involved, because now it is under Marshall management and he has to comply with Marshall documentation, which is different from Johnson. Just because he has something that qualifies at Marshall does not necessarily mean that it qualifies at Johnson and vice-a-versus. So, there on top of that, he is flying on three spacelab flights and there are three spacelab mission managers and all of them have their own ideas of what documentation should look like. So, he has had to prepare new documentation for each one of those flights. Well, the little thermal enclosure that Space Industries that I do consulting work for, they are going to start flying on the shuttle, the actual hardware cost a hundred thousand dollars to build. The delivered piece of hardware cost over a million dollars. So you have nine hundred thousand dollars worth of paper there to meet the Marshall requirements. I don't really think all of that is absolutely necessary.

Another case in point; we had an experiment that flew perfectly well. In fact, got some of the best results of any of the materials. It was really a Lewis experiment that flew on Spacelab 3 which that John Hart did. The original piece of hardware was suppose to have cost seven hundred and fifty thousand dollars and was built by Aerojet. I think they finally did bring it in for close to a million dollars, which is a pretty cheap piece of hardware, considering what we have had to pay for other pieces of hardware. The hardware functioned flawlessly, did a beautiful job. We wanted to refly the thing on USML-1, but now to update the documentation for the new requirements that got imposed on us after Challenger, we would have had to of spent over a million dollars just for new documentation to fly an existing piece of hardware. We decided that we couldn't afford it.

Now that is the kind of nonsense that we have gotten ourselves into. I don't see how you are ever going to do anything useful in this program, when you are spending those kinds of hours on hardware. Just to give you a few other horror stories. This may cook my goose politically. The point is I feel very strongly about this. In fact, (it) was one of the reasons I left NASA because I just didn't feel right about spending taxpayer's dollars this way. We flew experiments that we flew in Spacelab 3 wound up costing forty-eight million dollars. We grew about one millimeter of crystal which really could not be analyzed. We are going to refly it again on the USFL. Hopefully this time we might get a published paper out of it. We fly a drop dynamics module on Spacelab 3, Taylor Wang. The power supply failed in flight, but was actually repaired on flight by Taylor. That is one of the big arguments that Bobby likes to make about the presence of man, because, if it had been an unmanned flight, there would have been nothing to do, but to write it off. But the fact that Taylor was up there, he was able to go in and do the experiment well enough and was able to work around the problem and actually fix it and get about seventy-five percent of his data. You would think that NASA would herald this as something that, "Hey, this a really a good thing that we had a man up there." But a lot of people didn't look at it that way. In fact, I remember our infamous or famous ex-Deputy Administrat~~ion~~or, I can't think of his name, I remember his statement of, "Hell, every time I turn on the television all I see is Taylor Wang's ass sticking out of the rack, working on a piece of equipment." The JPL people weren't real happy about that because they thought it reflected badly on their hardware design.

Well, to make a long story short, when I was at headquarters, the JPL people came to us and said, "Hey, it take (We were going to refly the thing and finish Taylor's experiment) in order to upgrade this thing to Class B hardware (class A being mission critical, Class B being really having a lab on it and I think it was originally built as Class D hardware) is going to require twelve million dollars." This was another piece of hardware that was built, when we ordered it was only a million. But have we got a deal for you! What we could do is for fourteen billion, we built you a piece of hardware that will go all the way to Space Station. We are going to upgrade this stature capability and do all these wonderful things with a thousand whistles on it. I said, "Hey man, how could I pass up a deal like that. For only two more million dollars I can go first class, hey, let's go do it." Well, the year that I spent at Headquarters, that fourteen million escalated to somewhere around twenty-five million I believe. At last count, I believe it was up to seventy-five. They have had to degrade the capabilities of the equipment. Originally, they were going to be able to add high-temperature processing. They found that they couldn't really do that and meet with the delivery schedule. So, we have spent on that somewhere between fifty and seventy-five million dollars to basically design a piece of equipment that has the same capability of what we had years ago. It still doesn't do all of Taylor's experiments. There is no future in a program that costs that kind of dollars that produce some of the marginal results that we have gotten out of it. I mean it is just doomed to failure.

So all of these things, when you start looking at what it costs or what it would cost, to put this kind of hardware in space station and I can see why some Congressional people are going to have heartburn about this. We are talking to take a furnace that we basically already have, which again, Teledyne was suppose to build for an original six million and then twelve million, now it is up to thirty-five million for a simple furnace. It's not so simple, but it is still a furnace. Then to move that up to the Space Station, I mean, their budget is something like sixty million. This is totally absurd. The science return that we can get from these experiments, can in no way justify that kind of expenditure. Especially when you go up and maybe do one experiment every five or ten years. You don't advance science that way and the kind of dollars that we are putting into hardware just simply can't be justified with science return.

That is the other thing, I think, that NASA has a real cultural problem, because they are used to dealing with the astronomy community. Again, I think the astronomy community, frankly, has really gotten spoiled, because they have gotten use to big science budgets. When you stop to think about it, Lynn Fiske's budget for Space Science is over two billion dollars a year now. That is equivalent to the entire National Science Foundation budget. That is primarily just space science. Now you have to ask yourself, is it worth, as nice as it is to have pictures coming back from Venus and Jupiter and all these things, but do you really want to put half of your total government subsidized science budget just in space. I mean, there are a hell of a lot of other problems on earth that could be looked at. Things that are going to probably reflect more national competitives and align us, at least in economic terms, with West Germany, Japan, Korea and a few other places. Maybe we could argue that we are spending a hell of a lot more than we ought to be on ~~astatary~~ astronomy science. But you have a very strong lobby group in astrophysicists community that...there really is some absolutely fantastic science return from that and there is no other way to get that science return. But the materials problem is such that you can do a hell of a lot with that kind of money on the ground and finding ways to work around, rather than spending that kind of bucks in space. The NASA program managers have absolutely no feel for the value of the dollar in scientific world. To them, hey, if I need to make a modification and it is going to cost five million dollars to meet this schedule and to color in this mark, hey, let's spend it.

92. WARING: That is the heart of the problem then, that the NASA administrators and managers are isolated from economic consideration and scientific issues?

93. NAUMANN: I think that NASA has a real cultural economic disconnect. I don't think that they understand how difficult it is in the science world to get funding for projects that are really going to produce something that could be very useful and what those cost, versus the way that NASA throws around in "the name of science." You see, the worse thing of it is, if they were spending this two billion dollars a year on science, that would be one thing. But they are really not. A very large part of their money is going into aerospace contractors that are getting paid somewhere between close to two hundred dollars a year fully loaded, to do nothing more than push paper around. Where is the productivity in that? My god, we are going broke as a nation and we are still doing this kind of nonsense. You look at Space Station, we are burning...[end of second side of first tape]

But seriously, I think that it is not just NASA. I think that the whole nation has gotten into this thing.

94. WARING: Yes, too much bureaucracy.

95. NAUMANN: There are too many people sitting behind their PC's, because it is too easy now, behind a microprocessor to generate more paper and we have equated paper with productivity. Damn it, that is not anything that you can really sell.

96. WARING: Do you think that part of the problem is that from the very beginning, NASA has been involved in these engineering extravaganzas like going to the moon, and there are political goals that they are trying to achieve....

97. NAUMANN: I think that they have lost sight of reality. In fact, who was it recently that was talking about Mission to Planet Earth and one of the Congressmen said what NASA needs is a mission to reality! I think that is true, I really think that is true! Maybe the Space Station fiasco that they are going through right now, I don't know if that is going to jar them back into some sense. But you know, really, if NASA had to account for their dollars that they spend with their contractors in the same way that any business would have, in fact if the government as a whole had to do that, we certainly wouldn't do the things that we are doing.

98. WARING: Well, that certainly is a very interesting perspective.

99. NAUMANN: I think they just have a tremendously overblown concept of what the value of what they do really is.

100. WARING: Well, and the value of what complicated technology is. The Soviet example is one of using a lot of older technology...

101. NAUMANN: Yes, but they have managed to go broke to. I am not sure that is problem, I used to think it was!

102. WARING: Yes, you are right. Well, is there any stuff that we haven't talked about that I should know about. I should talk to Roger Chas~~t~~Say. Well, this last part is obviously more just for perspective.

103. NAUMANN: Yes, I would be a little bit concerned, frankly, if I was quoted as making some of those statements.

104. WARING: Okay, we will make that...

105. NAUMANN: After all, I still rely on Marshall for contract dollars and I still have a lot of good friends there and relations. But there is some serious concerns that need to be addressed.